1896MNRAS..56..299.

ADDRESS

Delivered by the President, A. A. Common, LL.D., F.R.S., on presenting the Gold Medal to Mr. S. C. Chandler.

The Council have awarded the Gold Medal to our Associate, Mr. Seth C. Chandler, "for his discussion of the variation of latitude, his work on variable stars, and other astronomical investigations"; and I have now to state to the Society the considerations that prompted this award, which happily I can do by a very brief review of the work of our medallist.

Before doing this I must express my regret that this chair is not occupied by one more familiar with the class of work under review, by one who could have subjected it to that critical comment to which the President's address on this occasion is so often devoted, and which I am sure the work in question would so well bear. Such criticism of our medallist's work is perhaps less needed on this occasion, as it has already been subjected to the competent examination of three of our Associates who are also medallists (Newcomb, Gould, and Asaph Hall), who signed the report of the Watson Trustees to the American Academy of Science, when the Academy in 1894 awarded the Watson Medal to Mr. Chandler. The fact that my own work is on other lines does not preclude me from expressing the great pleasure it has been to read again more carefully, for the purpose of this address, the many papers containing Mr. Chandler's contributions to astronomy, and my admiration of his work, particularly of that laborious portion resulting in the discovery of the law of variation of latitude.

This question of the variation of latitude in its present aspect dates back only a very few years. It was first noticed in our Annual Report in 1891, but there is no note in the 1892 Report. By the date of our annual meeting in 1893, however, the chief discovery of our medallist (the variation in a period of 428 days) had been made, and eight or nine papers had already appeared of that brilliant series of researches by which the complete law of variation has been practically established. This series, including investigations arising from the main inquiry, has been continued to the present time; and the consequences of Mr. Chandler's discoveries are so far-reaching, and his energy in following them up is so unfailing, that we may hope for equally interesting

1896MNRAS..56..299.

results in the future. But the work has already reached a stage of completeness in that the main investigation is concluded. Our medallist has sufficiently elucidated the law of latitude variation to publish an ephemeris of it. In his own words, "The relative motion of the earth's axis of figure and rotation is governed by a law, synthetically derived from observation, whose constants are so accurately known that we can with certainty compute in advance tables of the variations of latitude for any given station," and he proceeds to give such tables for the years 1893–96 for which "a convenient and suitable place in the future would be the various astronomical ephemerides." Remembering that the variation of latitude was not much more than suspected some five years ago, and that there was not even a suspicion of the true *law* of variation (except, perhaps, in the fertile brain of our medallist), the proposition to now print such tables of prediction side by side with tables of the planets is somewhat startling. But a careful examination of Mr. Chandler's work forces upon us the conviction that his confidence in it is justifiable. In the past four or five years he has found time to discuss, exhaustively for the present purpose, all the observations available from Bradley to the present time, so that as regards their foundation his ephemerides are well worthy of a place beside others; while the close accordance of results derived from such a large number of independent sources is a guarantee of the accuracy of the law finally adopted.

The history of these investigations is most interesting. More than a century ago it was shown by Euler that if the axis round which the Earth revolves be not coincident with the axis of figure, the position of the former in the earth would shortly change, revolving round the axis of figure in 305 days. Euler's work started with the assumption that the Earth was a perfectly rigid body, which is not the case, and we shall presently see how this assumption affects the result; but it never occurred to anyone until a year or two ago to consider the effect of non-rigidity. Euler's result was accepted, and search was made for indications of this motion of the axis of rotation in a period of 305 days, or ten months; but no such motion was found, though several investigators tried to detect it. The first to make the attempt, though unsuccessfully, was C. A. F. Peters, then at Pulkowa. Negative results were also obtained at Washington, at Pulkowa again by Nyrén, and by Clerk-Maxwell and Downing from Greenwich observations. It was thus supposed that the axes of rotation and figure were sensibly coincident, and little further interest was taken in the matter. The first fifty volumes of our *Monthly Notices* contain no reference to the variation of latitude beyond the paper by Downing, of two or three pages, in which he shows that ten years' Greenwich observations of *Polaris* show no 305 day variation.

The question was definitely reopened by Küstner, of Berlin. In 1885 he remarked that some curious discordances affecting

1896MNRAS..56..299.

Nyrén's observations with the prime-vertical transit could be accounted for by supposing a change of latitude; and in 1888 he published a memoir on the constant of aberration, as deduced from a series of observations made by him at Berlin in 1884–5, from which it appeared incidentally that the latitude of the observatory must have changed during the period of observation. So conclusive was this demonstration of change that special attention was forthwith directed to the point by the International Geodetic Association. Under its auspices special observations were organised to detect any changes in latitude at three or four observatories, with immediate and conspicuous success. I cannot here follow the course of this observational work, which has proceeded continuously up to the present time, and which it is now proposed to extend and develop further. The investigations of our medallist have been conducted to a large extent independently of it (though latterly he has made excellent use of the valuable material resulting from these observations), and we are to-day concerned especially with his work alone.

As above stated, it was Dr. Küstner who first called the attention of the astronomical world to the variation of latitude; but similar ideas had occurred to Mr. Chandler at about the same time, though circumstances prevented him following them up. In the years 1884–5 he made a thirteen months' series of observations with the almucantar (an instrument of his own devising, of which I shall presently speak), which revealed a progressive change in the value for the latitude given by this instrument, and he was unable to refer this apparent change to anything instrumental. As he says later, he hesitated then to ascribe it to a real change of latitude without further investigation, which he was not able to undertake at the time. Six years later, when the change had been meanwhile strikingly confirmed by the publication of Küstner's observations (also in 1884–5), and general attention had been called to the matter, Mr. Chandler was able to resume the inquiry, and his success in finding the key to the solution of the problem was almost immediate. His work is detailed in some score of papers in the *Astronomical Journal*, following one another with marvellous rapidity, considering that the amount of work represented by each was in general undertaken and completed in the interval since the last. For when sure that he had got hold of the real thread, Mr. Chandler began to publish at once without waiting to unravel the tangle completely. As he cleared each successive length, he presented it to the world; and this method gives a freshness to the resulting series of papers which can only be realised by reading them in the original.

The first important step, the detection of the fourteen months period in the latitude, resulted from an examination of the Pulkowa vertical-circle observations, 1863–1875, and the Washington vertical-circle observations 1862–1867. This periodicity was confirmed by examination of observations at Melbourne and

1896MNRAS..56..299.

Leyden at about the same time. Here was a singular discrepancy with Euler's theory; which had so definitely indicated a period of ten months, if any, that none other had hitherto been looked for. An explanation of the apparent conflict between theory and observation was, however, soon given by Professor Newcomb, who pointed out that a deviation from absolute rigidity in the Earth would alter the Eulerian period. More elaborate investigations of this point have since been made by Woodward in America and Hough * in England, and there seems little doubt that this explanation, fortunately so quickly forthcoming, is the true one. But the few months during which theory and observation were not reconciled had a marked effect upon the future course of Mr. Chandler's work, which was from that time characterised by a stern resolution to set aside any teaching of adopted theories and to let the observations speak for themselves. I do not mean to suggest that this resolution was born after the beginning of his researches, for it can be traced in his first papers; but it was undoubtedly much strengthened by the events of these few months, during which his results were regarded by many as very questionable, owing to their want of accordance with a theory afterwards shown to be defective. And his resolve stood him in good stead, for his deductions from observation were soon again at variance with theory. On examining earlier observations in Bradley's time he found that the fourteen months' period had disappeared, and in its place was a variation in twelve months of larger amplitude: and there were indications of a progressive change from one period to the other. These results, promptly and fearlessly announced, brought a new wave of scepticism, and almost carried back those who had been won over (like Professor Newcomb) into the opposition camp. Fixed in his determination, however, to be undismayed by any apparent conflict with theory, our medallist went on patiently examining huge masses of observations wherever he could find them suitable for his purpose. The situation is so dramatic and the study of it such an incentive to true scientific investigation that I make no apology for re-

---

* In a paper communicated to the Royal Society last month, Mr. Hough gives an analytical investigation of the motion of simple rotation about a principal axis, taking into account elastic distortions due to variations in centrifugal force; the results are found to agree in the main with those obtained by Professor Newcomb from geometrical considerations. The analysis deals with the case of a homogeneous spheroid of revolution, the ellipticity being such that the body is free from strain when rotating uniformly. Such a spheroid, if of the same size and mean density of the earth and rotating with the same angular velocity, would oscillate in a period of 232 days if perfectly rigid; it is shown that this period would be extended to 335 days in virtue of elastic distortions if the rigidity were equivalent to that of steel. In the case of the Earth the period would be still further prolonged in consequence of variations in density, and the period which corresponds to the above degree of rigidity is estimated at about 440 days; whence it is concluded that the observed period may be accounted for by supposing that the Earth is capable of elastic deformation, and that its effective rigidity is slightly in excess of that of steel.

1896MNRAS..56..299.

calling Mr. Chandler's own statement of it, which has already been quoted in the Report for 1893 :—

"It should first be said that in the beginning these investigations I deliberately put aside all teachings of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself; and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever. . . . The appeal to observation, treated irrespective of theory, in the present series of papers, shows that a rotation of the pole really exists, but (*a*) at a daily rate of but 0°·85 (for 1875), and (*b*) that this velocity is subject to a slow retardation, which, in its turn, is not uniform. . . . The result (*a*) was at first pronounced impossible, and is even now so regarded in some quarters. Professor Newcomb, however, soon after found the defect in the theory, and is now as cordially in favour of the result given by observation as he was originally against it. . . . Now, may it not reasonably be asked if the direct deduction from observation has led to the correction of theory in the first particular, is it beyond hope that it may do so in regard to the second ?"

Six weeks after the publication of these words "theory and observation were again brought into complete accord," but this time by Mr. Chandler himself. By re-arranging his material he found that the variation of slowly changing period and amplitude which had scared away his theoretical friends was really a superposition of two variations, one in fourteen months and the other in twelve, one of which gradually overtakes the other in the familiar manner. He has gradually built up the complete description of this compound motion, and has shown that the former component is due to a circular revolution of the pole of the axis of figure about the pole of rotation in a radius of 14 feet: and the latter to the motion of the pole of figure in an ellipse 25 feet long and 8 feet broad, the major axis being inclined at present about 45° to the Greenwich meridian. There are indications of slow changes in these elements, and other details on which I need not here dwell. Rather would I call your attention to one or two general characteristics of Mr. Chandler's work, which stamp it as in every way worthy of the honour now conferred upon him.

I have already mentioned his determined independence of preconceived hypotheses, and his resolution to appeal to the facts themselves, and to them alone. An equally striking feature of his work is the enormous mass of it, of which it is difficult to give an adequate idea. The fact that in the last five years he has contributed fifty papers to the *Astronomical Journal*, covering in all 150 pages, is in itself noteworthy ; but many of these papers represent in a few pages an amount of work which it is

1896MNRAS..56..299.

almost impossible to estimate. Paper No. VI. of the series on variation of latitude, for instance, gives the results of the examination of forty-five different series of observations at different observatories. Three other papers of the above fifty are the Second Catalogue of Variable Stars and its two supplements; and a short note, of only half a page, announcing the recent discovery of a variable period of 5½ hours, represents a large amount of work at the telescope. Throughout the absorbing researches on latitude Mr. Chandler never lost touch with variable stars and comets, on which he had laboured for many years; and I might emphasise not only the amount but the variety of his work. Yet I trust I may not appear fanciful in suggesting that in this variety of work there is a unifying principle, for it seems to me that Mr. Chandler's genius for the detection of a new period is manifest throughout. His work on variable stars led up to his great discovery in the variation of latitude. The two Catalogues of Variable Stars with their supplements, which he has published, are no mere compilations, but represent the critical scrutiny of all available observations; the correction of many elements and the revision of others; the detection of waves subsidiary to the main light-curve; and, finally, several actual discoveries by the author himself. Such work as this trained his natural delicacy of perception of periodicity, and he became an expert, thoroughly competent to deal with such problems. To detect an existing periodicity or to trace the true cause of abnormal observations may seem an easy matter to those who have not tried it, but history proves the contrary. Thirty-nine years ago our Medal was awarded to M. Schwabe for his discovery of the periodicity of solar spots. The occupant of this chair then cited several authorities to show that for a century previous to the commencement of Schwabe's work no suspicion of periodicity had been awakened—nay, more, that any systematic regularity in the appearance of sun-spots was categorically denied. Schwabe's discovery was only made as the result of thirty years' unremitting labour at the telescope, though it is difficult for us now to imagine how it could have been overlooked by the most casual observer, so different is the situation before and after the discovery. A minor instance of the difficulties attending the detection of a true period has recently been afforded by our medallist himself in the case of his new variable of 5½ hours period.

"It may not be amiss to add," he says, "that I discovered the variability of this star more than a year ago, but erroneously inferred that it was of the Algol type, with a period of 2·06 or 2·07 days; a mistake due to the near commensurability of nine of the true periods with two solar days." Thus the star had run through its changes some 1,500 times while under careful scrutiny before its true period was detected. Most of the time it was, of course, not available for observation owing to daylight and cloud: and this necessary intermittence in the observations with the

1896MNRAS..56..299.

masking of the period by commensurability with others, as above-mentioned, are two of the chief difficulties in such work. We have recently seen a remarkable illustration of these difficulties in the controversy respecting the rotation periods of *Mercury* and *Venus*. These planets can only be observed intermittently at intervals of about a day, and it is not yet established beyond question whether their rotation periods are commensurable with the mean solar day or with their periods of revolution round the Sun. It is difficult to imagine what would have been the case with that greatest of modern discoveries—the finding of *Neptune*—if a working hypothesis in the shape of the assumed elements indicated by Bode's law had not existed, or how long another important discovery, that of argon, and indirectly of helium, would have been postponed, if the anomalous observations of the weight of nitrogen had not been traced to their true cause by Lord Rayleigh.

So far I have spoken chiefly of Mr. Chandler's great discovery of the true law of variation of latitude; and this address has already run to such a length that his other work, sound and original as it is, must be reviewed very briefly. His Catalogues of Variable Stars, and his observations and discoveries in this department of science, have already been mentioned, and though much might be said in their praise, I must reluctantly forego this privilege on the present occasion. I would, however, claim your attention for a few moments to the instrument devised by Mr. Chandler, and called by him the almucantar, embodying as it does a new principle applied with great success to an observing instrument of precision.

A full account of this instrument, with the methods of using it and the observations made, extending over a period of thirteen months, forms Vol. XVII. of the *Annals of Harvard College Observatory*. It is the outcome of an idea which occurred to Mr. Chandler that another method of making that class of observation for which meridian instruments are used might be accomplished by substituting for the meridian as a fundamental plane of reference the small circle perpendicular to the meridian passing through the pole, and for the motion of rotation determined by the pivots of a horizontal axis, one determined by gravitational action round an imaginary vertical axis. Two ways suggested themselves of carrying out this object: to suspend the instrument like a pendulum, or to float it on mercury. Both were tried, but the latter plan, as involving fewer mechanical difficulties, was adopted for a small trial-instrument of $1\frac{3}{4}$-inch aperture and 25-inch focal length. With this instrument and method results of a surprising degree of accuracy were obtained—for instance, the latitude of the Harvard College Observatory as determined with it was eventually found to be more accurate than that determined by the fixed instruments of the observatory.

Of the larger instrument subsequently made, which forms the subject of the memoir already mentioned, it is not necessary

1896MNRAS..56..299.

for me to say more than that the discussion of the observations made with it shows that it is a most valuable instrument, capable of an accuracy equal to, if not exceeding, that of larger fixed instruments.

One particular point I would like to mention is the marvellous accuracy with which the telescope comes to its true place after each fresh setting. As you no doubt are aware, the observing telescope, and the horizontal axis on which it moves in a vertical plane, are carried by a counterpoised float in a box of mercury, this box in its turn being supported on a vertical axis. The float with the telescope on it is quite free from mechanical connection with the box, the only restraint being that given by two pins working in upright guides to restrain movement sideways.

When the telescope is clamped on its axis to any altitude, the circle it would describe in the heavens if rotated on the vertical axis would be a small circle or almucantar; and the successful use of the instrument depends on the truth with which the telescope can be set to different points on the small circle to which it is at first pointed. Although between each setting the rotation in azimuth sets up a considerable amount of oscillation, the instrument settles down after a short time to a position so nearly the true one that it is difficult to determine its departure from it. In that part of the memoir speaking of this, Mr. Chandler says that after a couple of minutes a mean position of equilibrium is regained within a range of variation so minute that no means exist of measuring it with certainty. In Chap. V. it is experimentally demonstrated that the probable error of equilibrium must lie within one-tenth of a second of arc, corresponding to one hundred thousandth of an inch difference in the two ends of the float.

The consistent way in which this instrument behaves, and the remarkable accuracy with which it takes the true position after a change in azimuth, are due not only to the principle of floatation employed, but also to the peculiar simplicity of construction that this principle allows, for the fresh settings are obtained without the least change of strain in the instrument. In this latter respect it enjoys an advantage over the necessarily heavy meridian instrument with its puzzling flexure; the freedom from mechanical connection with the Earth gives it another hardly less important; while the cost of the respective instruments would be greatly in favour of the simpler form. It seems to me that we have in this instrument capabilities that have not yet been fully appreciated and applied.

It has often occurred to me how the personal character of the designer of an instrument is reflected in the design. We can certainly recognise the same independence of preconceived notions in the almucantar that Mr. Chandler shows in his theoretical discussions; and we find another point of similarity when we compare the speedy way in which the instrument in question takes up and keeps its true position after a few oscillations, with

1896MNRAS..56..299.

the manner in which, after a few tentative hypotheses, none of them far from the truth, our medallist found and maintained the real solution of the problem of the variation of latitude.

In this brief review of your medallist's work I think I have said amply sufficient to show you that in computational, observational, and instrumental astronomy, our medallist is in the first rank, and is, indeed, well worthy of the highest honour which it is in our power to bestow upon him.

Dr. Huggins, I place this medal in your hands, as Foreign Secretary, for transmission to Mr. Chandler. Will you express to him our high appreciation of his contributions to astronomy, and our hope that he may be long spared to further enrich our science.

Our regret that Mr. Chandler is unable to be present to-day is tempered by the expectation that some of our Fellows in crossing America for the observation of the Solar Eclipse will be able to personally express this appreciation and hope in a warmer way than can well be done in a letter.